Reviewer: Steve Carpenter
Title of Paper: *PATH Final Report for Fiscal year 1998*
Section: 1 (Exec Summary)

Comments:

a) scientific soundness of the methodology

The approach seems sound. It would be possible to clarify the presentation on some points (these are not issues of soundness, but of presentation). Specifically:

p. 5-6: Why not tell us what the 3 official NMFS standards are, instead of having us guess from clues scattered through the manuscript?

p. 6: Why use the lowest of the 3 probabilities to represent the overall probability? Of course, the 3 probabilities are not independent, so the product is not quite right, either. Is the minimum probability index mandated by NMFS?

p. 6-7: What is the rationale for basing recovery standards on the last 8 years of a run? If the 48 year standard is intended to represent some sort of long-term recovery, why not use the last 16 years of the 48 year run (i.e. the same fraction of total years as applied to the 24 year run)?

b) general suitability of the data for use in the analyses

Section 1 deals mainly with model results rather than data, but the data which are used are appropriate.

p. 12: For 'business as usual' simulations, are simulated SARs similar to historical SARs? The simulated SAR improvements from the various actions are hard to interpret unless one knows how well the historical simulations match historical SARs.

c) validity of inferences and conclusions reached

I found no major problem with validity. The manner in which conclusions depend on weights and professional judgements (as opposed to data, in cases where hypotheses are uncertain and data are unavailable) is apparent from the text.

d) suggestions for improvements and extensions to the analytical approaches used

None at this time.

e) opportunities for integration of the different component analyses into an adaptive management approach

This integration is already occuring in Section 6; see comments on that section.

f) relative priorities for future work on these analyses

Move on to selection and implementation of experimental management options.

Reviewer: Steve Carpenter
Title of Paper: *PATH Final Report for Fiscal year 1998*
Section: 2 (Spring/Summer Chinook)

Comments:

a) scientific soundness of the methodology

Methods appear sound.

b) general suitability of the data for use in the analyses

Data are appropriate.

c) validity of inferences and conclusions reached

Uncertainties are stated clearly and appropriately; conclusions are well supported by evidence and analyses.

d) suggestions for improvements and extensions to the analytical approaches used

None.

e) opportunities for integration of the different component analyses into an adaptive management approach

The fact that the actions create major splits in the CART diagrams suggest that further modeling or analyses of historical data are unlikely to resolve uncertainties. It is time to manipulate the system. Furthermore, the analyses presented here suggest that decision makers have an opportunity to simultaneously increase probabilities of meeting jeopardy standards while decreasing uncertainties.

f) relative priorities for future work on these analyses

Move to the implementation phase.

Reviewer: Steve Carpenter
Title of Paper: *PATH Final Report for Fiscal year 1998*
Section: 3 (Fall Chinook)

Comments:

a) scientific soundness of the methodology

The general approach is sound. There are many specific questions below. I cannot tell whether these issues are problems with methodology, or simply unclear from the information presented.

p. 93: Recruits (freshwater)- recruitment from which phase to which other phase? Is this recruitment to freshwater from the ocean?

p. 93: Conversion rate- What exactly is this? Fish that make it to spawn divided by fish entering the system at Astoria? More specific definitions of conversion rate are given later in the report but I am not sure how these relate to page 93.

p. 104: How were the Ricker fits done (Max likelihood, least squares? Assumptions about error sources? Diagnostics?)?

p. 118: Direct transport mortality is 98%? This sounds high. Is 0.98 a survival rate (not mortality rate)?

p. 120 top: What do you mean by "turning off" a parameter? Probably this means fixing it at some value, maybe zero, but setting some parameters to zero squashes any effects of other parameters (e.g. if beta_flow is zero, alpha2 and T_seasn are removed as well).

p. 123 bottom: "I would argue for . . ."—Who is 'I'? Is this the voice of the ESSA team?

p. 134-135, explanation of Eqn 3.2.2-2: DP_t appears in the equation but only D appears in the list of symbols. Is P_t a misprint for Pb_t? In the paragraph below the list of symbols, we get Dp_t and DPb_t. Either there are typos on the paragraph, or symbols missing from the list, or both.

p. 136 prior distributions: If the priors are uniform, then there were sharp cutoffs which disallowed parameter values outside the range of the uniform distribution. In this situation, the statement that 'parameter value is determined only by the data' cannot be true, because the prior excluded some parameter values. Perhaps a noninformative improper prior (e.g. a Jeffreys' prior) was used, which is something like an unbounded uniform distribution. If a uniform distribution was used, the limits should be stated.

p. 140 upriver survival: The paragraph needs a one-sentence definition of conversion rate (fish passing uppermost dam / fish entering at Bonneville ?).

b) general suitability of the data for use in the analyses

Data are appropriate.

c) validity of inferences and conclusions reached

Authors are careful to state that results are preliminary. Consequently I focused on issues of methodology and clarity of presentation that could affect conclusions, assuming that there will be a later opportunity to consider conclusions themselves.

d) suggestions for improvements and extensions to the analytical approaches used

The predator impacts could be large and variable (page 130). Two key predictors are predator biomass and temperature effects. Temperature effects are captured by the bioenergetics analysis, but why not make the equations (bottom p. 130) dependent on temperature? Perhaps temperature is fairly constant from year to year, but even a few degrees C can have a big impact on consumption by predators. The importance of temperature fluctuations could easily be checked using sensitivity analysis. Predator biomass data are very expensive to collect and we may have to live with whatever is available. The information presented on catfish is alarming and suggests that both the mean and variance of predator may be underestimated. The importance of this factor could be explored with sensitivity analysis.

p. 141: The hatchery supplementation assumptions need to be addressed by sensitivity analysis. The sensitivity analyses should include the hypothesis that hatcheries diminish returns, e.g. by disease, through genetic factors that make the overall population less resilient to future environmental changes, by allowing harvests to be sustained thereby driving down the ratio of wild-type to hatchery individuals in the overall population, or other plausible mechanisms.

e) opportunities for integration of the different component analyses into an adaptive management approach

The sensitivity of outcomes to the management options suggests that fall chinook, like spring chinook, are good candidates for adaptive management. A weight-of-evidence process would refine the weights on the alternative hypotheses, but it is likely that significant uncertainties will remain and that the predicted outcomes will be sensitive to the management actions. Within the near future, we will have gone as far as we can go with modeling and analysis, and action will be the only option.

f) relative priorities for future work on these analyses

Sensitivity analyses noted above, and others mentioned in the report, should be conducted.

A WOE process would refine the weights on the hypotheses. Also, it would help guide the choice of experimental actions, by providing information about which actions are most likely to meet the NMFS goals.

The spring chinook case may forecast what might be obtained from a WOE process. For spring chinook, how would management choices and experimental designs change for agnostic weights (50:50) versus the actual WOE weights? To what extent are spring chinook WOE weights transferable to fall chinook?

It is critical that we continue to work toward action designed to meet the NMFS goals while at the same time reducing uncertainties by testing key hypotheses.

Reviewer: Steve Carpenter
Title of Paper: *PATH Final Report for Fiscal year 1998*
Section: 4, Steelhead

Comments:

a) scientific soundness of the methodology

The qualitative approach proposed for the steelhead analysis is far less rigorous than the PATH approaches for chinook. This is due principally to the paucity of data on steelhead. The approach is valuable, because it raises a wide range of issues relevant to the question: Do policies that meet goals for chinook have a high probability of meeting similar goals for steelhead? However, I suspect that the answers to questions 1-7 (p 172) will end up being very soft. This could be OK if it helps policymakers understand that any predictions on steelhead are very uncertain.

b) general suitability of the data for use in the analyses

As far as they go, the data are suitable. It's important to repeatedly stress the limitations of the data.

c) validity of inferences and conclusions reached

The conclusions stated on p. 199 seem arguable to me. Although I am not an expert on these particular stocks, the qualitative approach and the paucity of data suggest that definitive answers will be lacking. Thus I am surprised to see answers as unequivocal as "yes" on p. 199.

d) suggestions for improvements and extensions to the analytical approaches used

Why not stress the uncertainty and lack of information?

e) opportunities for integration of the different component analyses into an adaptive management approach

Steelhead would seem to be an important element of any experimental management. The information base is so weak that substantial improvements are possible from adaptive management. On the other hand, the weakness of the information base may make it difficult to pose alternate hypotheses to guide experimental designs.

f) relative priorities for future work on these analyses

It seems crucial to move beyond the qualitative approach to analyses that lead to alternative hypotheses which could be examined by experimental management.

Reviewer: Steve Carpenter
Title of Paper: *PATH Final Report for Fiscal year 1998*
Section: 5, sockeye

Comments:

a) scientific soundness of the methodology

The document is sound as far as it goes. It seems important to stress the preliminary nature of the analysis, as stated on p. 207.

b) general suitability of the data for use in the analyses

Data are sparse but appropriate.

p. 212: The paragraph above figure 5.2.1-2 describes patterns that are very difficult to see in the data. The most striking patterns in the data are the increase from 1939 to the mid-1950s, and the sharp decline about 1995. Yet the paragraph focuses on the mid-50s to mid-90s, where noise makes it impossible to see any pattern!

c) validity of inferences and conclusions reached

Essentially, no major conclusions are reached and this is appropriate.

d) suggestions for improvements and extensions to the analytical approaches used

Data limitations seem to be the most serious problem.

e) opportunities for integration of the different component analyses into an adaptive management approach

Like steelhead, sockeye may prove to be an important candidate for experimental management. Because so little is known, it may be possible to substantially reduce uncertainties by experimental management.

f) relative priorities for future work on these analyses

It seems like the lack of well-grounded alternative hypotheses could become an impediment to learning from experimental management. Thus it seems important to advance the analysis to the point where key alternatives are clear.

Reviewer: Steve Carpenter
Title of Paper: *PATH Final Report for Fiscal year 1998*
Section: 6, Experimental Management

Comments:

a) scientific soundness of the methodology

This is an excellent summary of adaptive management with some preliminary thoughts on possible experimental designs gleaned from the Sept 1998 workshop.

b) general suitability of the data for use in the analyses

The experimental designs discussed are well grounded in the uncertainties and alternative hypotheses developed for spring chinook.

c) validity of inferences and conclusions reached

On p. 222, table 6.3-1, 2-pool drawdown, I did not understand why the next-to-last design was considered most risk-averse. Couldn't the first design, with experimentation on both hatcheries and transport, be equally risk averse? Maybe risk aversion needs to be quantified through model runs.

d) suggestions for improvements and extensions to the analytical approaches used

p. 219 bottom: This paragraph really makes 2 points, one about passive versus active management and the other about various time series methods for interpreting unreplicated ecosystem experiments. These points should be addressed separately.

First, I have repeatedly stressed the importance of strong, sustained manipulations for ecosystem experiments. This is an active approach. Carpenter (1998) is a good summary of these arguments. For analyses that address treatment strength for specific proposed ecosystem experiments in midwestern lakes, see Carpenter (1989) and Carpenter et al. (1995). The general argument for strong, sustained manipulations is independent of the particular statistical approach chosen for any given experiment.

Second, I would not rule out the possibility of using time-series approaches for adaptive management experiments in the Snake/Columbia system. In some cases, substantial baseline time series already exist. The original paper by Box and Tiao (based on passive adaptive data) obtained insights from relatively short time series. With active approaches, one may have even more sensitivity (Carpenter 1993). Finally, clever choices of reference ('control') ecosystems can sometimes improve the ability to discriminate alternative models in ecosystem experiments, using relatively short time series (Carpenter et al. 1998). Other streams in the region, or other tributaries to the Columbia that enter downstream of the Snake, may provide informative reference ecosystems.

It is important to note that some responses to adaptive management in the Snake/Columbia will be "one-way trips" and Box-Jenkins type time series methods will not work. This is not an unfamiliar problem in fisheries analysis (Hilborn and Walters 1992). The "one-way trips" will pose a special set of challenges for comparing alternative models. In other cases, however, baseline data exist and stock responses may show a "two-way trip" (down, then up). It may prove easier to discriminate alternative models for these cases.

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e) opportunities for integration of the different component analyses into an adaptive management approach

That's the whole point of this section.

We must be cautious not to promise too much too fast. For example, the top of p. 220 states that studies taking many decades are inconsistent with salmon recovery. Delaying many decades is inconsistent with salmon recovery, but certain experiments initiated promptly are consistent with salmon recovery. Even if we initiate recovery now it could take many years to learn. We should be acknowledge that clear evidence for and against various hypotheses could take a decade or more to develop.

f) relative priorities for future work on these analyses

The general plan laid out in the Section seems appropriate.